THE BOOK WAS DRENCHED

THE EVOLUTION
OF
MATHEMATICAL PHYSICS

Being the Rouse Ball Lecture for 1924,

BY

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CAMBRIDGE
AT THE UNIVERSITY PRESS
1924

PRINTED IN GREAT BRITAIN

THE EVOLUTION OF MATHEMATICAL PHYSICS

THE founder of this Lecture has chosen as one of his special interests the history of Mathematics, both through the ages and as reflected in the studies of the University. Within a short compass he has given an account of the development of the subject which contrasts with the elaborate treatises of previous writers by its concentration on essentials, and also by the glimpses which it affords of the personalities of the mathematicians whose achievements he records, with their limitations and their failures, as well as their ambitions and successes.

The study of the successive steps in the evolution of any subject is an attractive pursuit, and many years ago, speaking not far from this place, I was led to hazard some speculations as to the ideas which prompted and guided the very first steps in the development of Greek Mathematics. I even ventured to say, not altogether in the spirit of paradox, that if any one scientific invention could claim pre-eminence above all others, I should be inclined to suggest a monument to the unknown inventor of the mathematical *point*, as the first step in that long process of abstraction and idealization which has culminated in the science (and not merely mathematical science) of to-day. I remember that the eminent engineer who sat near remarked to me afterwards that if the scale of subscriptions was to be appropriate to the dimensions of the object to be commemorated he would gladly head the list. An even more eminent astronomer told me that the whole address was an elaborate scientific joke. Such friendly satire did not disturb my opinion; but speculations on the psychology of the primitive mathematicians, attractive as I think they are, are necessarily precarious, and I am not tempted to venture on this field again. The task which I would attempt to-day is to trace the leading steps in the development of that great tradition of Mathematical Physics, as distinguished from Dynamics and Astronomy, which began in the early years of the last century, and has dominated physical speculation until quite recent times, when new discoveries and new ideas have emerged, calling for newer methods, without, however, rendering the old ones obsolete. The ground has of course been traversed before, but not I think quite from the present point of view. I am not concerned with physical theories as such but rather with the mathematical dress which they have assumed from time to time. My object is to shew how it comes about that we have inherited a mathematical scheme which in its final form embraces subjects physically so different as Heat-Conduction, Hydrodynamics, Elasticity, Magnetism, Electricity, and Light, and can be made to include any one of these by assigning proper names to the symbols. The scheme admits of course of being set forth in a purely abstract form without any physical reference at all, and this has in fact been done; but its chief value is for the physical analogies which it facilitates, and in which it originated. The development has been continuous, although the wide scope of the final result could not have been foreseen.

The time I have indicated as a starting point was peculiarly favourable. The great calculator Euler had ranged over the whole field of Mathematics, and had given to many parts of it almost the final form which we find in our text-books. Lagrange, Laplace, and Legendre had developed the Newtonian Astronomy, and made important contributions to general Dynamics, as well as incidentally to Analysis. So that when attention began to be directed to physical subjects the available mathematical resources were far in advance of what had been within reach at any earlier period.

Isolated questions of course had been treated previously; for instance the flexure of bars had been discussed by Bernoulli and Euler. More important from the present point of view is the foundation of Hydrodynamics by Euler, who formulated the fundamental differential equations, and proceeded to integrate them on the supposition that a velocity-potential exists. He was careful to note, however, that there are cases, such as that of uniform rotation about an axis, where this condition is not fulfilled. The theory of plane waves of sound was also known, and I need hardly recall the subject of vibrating strings with its reactions on Analysis, and the long controversies which resulted. But the starting point of Mathematical Physics, in the now general sense of the term, is to be fixed I think about the time when the storms of the French revolution had subsided and were succeeded by the comparative tranquillity of the early Empire. If a more definite date is required, we may perhaps fix on the year 1807, which was marked by the publication of Poisson's first memoir on Sound. This deals with spherical waves, with waves in an atmosphere of variable density and, most astonishing of all, with waves of finite amplitude. He finds that the boundaries of such a wave advance with the ordinary velocity of sound, but omits to examine the progressive change of type. This was only done long afterwards by Stokes. It may I think be said of Poisson that, with all his extraordinary power in dealing with a problem when once it had been reduced to an analytical form, and the great achievements which stand to his credit, he was less concerned with the physical interpretation of his results.

The same year, 1807, is still more memorable for the first instalment of Fourier's investigations on the Conduction of Heat, whose importance extends far beyond the special subject. Mathematicians so eminent as Hamilton, Maxwell, and Kelvin have found it difficult to speak of Fourier in measured terms of appreciation, whether of the ingenuity of his mathematical processes, the elegance of his results, or of his broad and philosophical outlook, as revealed especially in the preface to his formal treatise. Fourier had indeed the advantage of a rather varied career. He was trained at first for the priesthood, then rejected for the (royalist) artillery school, with the remark in so many words that the lowliness of his origin would have disqualified him "even if he had been a second Newton." He became a pupil at the École Normale, and later professor at the École Polytechnique. He was included in Napoleon's expedition to Egypt, as a Member of the ambitious Egyptian Institute which it was proposed to found, and of which Monge was President. Returning to France in 1802 he was made prefect of the Department of the Isère, possibly on account of the administrative talent which he is said to have displayed in Egypt, and it was at Grenoble that he began the composition of his classical work. His subsequent history, though interesting and honourable, hardly concerns us, but the facts I have mentioned suggest that his varied and responsible experience, as well as the literary studies which were an obligatory part of his early education, and in which he is said to have excelled, was not without influence on his work, or on the luminous style in which it is explained.

At the very outset of his book we meet for the first time with a process which now seems so obvious and familiar that the mention of it may appear trivial. I mean the device by which the rate of change of a physical property at any point of a medium is calculated in terms of its flux into an element of volume. But it could hardly have been quite obvious, for many years elapsed before so simple a matter as the equation of continuity in Hydrodynamics was proved in this way by William Thomson, who also pointed out its utility in the expression of Laplace's equation $\nabla^2\phi = 0$ in curvilinear co-ordinates. At a later period the process received a brilliant extension at the hands of Maxwell, in his theory of gases, where it was applied to the flux of momentum and also of energy.

The mathematical methods employed by Fourier in his treatment of special problems repay a careful study. As they stand they would often fail to satisfy even a lenient standard of mathematical rigour, and indeed they appear to have raised doubts in the minds of Laplace, Lagrange, and Legendre, who formed the distinguished commission charged to examine one of his memoirs. But they are models of what may be called mathematical experiment; and at any rate they are successful in the end, and the results are easily verified. The form, again, in which these results are presented is I think quite unlike anything that had gone before, especially in the occurrence of definite integrals, but a slight examination shews that it would be difficult to imagine anything more adapted to the particular circumstances, or really more lucid. One special question examined by Fourier may be noticed for its connection with more recent speculations. It had been debated whether the earth has an intrinsic store of heat, or whether it was altogether dependent on the sun. Fourier's conclusion is that the internal temperatures are independent of the solar influence, but that the latter is mainly responsible for the superficial temperatures. Among Fourier's anticipations of modern practice, we may cite his recourse to graphical methods for the solution of equations, and especially his insistence on the necessity that results should be capable of reduction, when needed, to numerical form.

The general equations of Hydrodynamics date from Euler (1755), but a long period elapsed before any but the simplest applications were made of them. The theory of waves on water was propounded by the French Academy as the subject of a prize essay for the year 1815. The problem proposed was to trace the effect of a given initial disturbance of the surface. The memoir of Cauchy, to whom the prize was awarded, is remarkable as containing the first satisfactory proof of the persistence of the irrotational quality in a portion of fluid which possesses it at any one instant. The analytical difficulties of the special problem are considerable, owing mainly to the fact that there is no definite wave-velocity, but the genius of the author supplied what was wanting, and the notes afterwards appended to his memoir contain a store of important analytical results, relating chiefly to definite integrals. In particular we meet here for the first time with the integrals known afterwards by the name of Fresnel, who encountered them in his work on Physical Optics. A parallel and independent memoir by Poisson, who was himself debarred from competing for the prize, confines itself more closely to the terms of the problem, but agrees in the main results. It is remarkable that neither writer pauses to consider the simpler and more fundamental properties of a simple-harmonic train of waves. This was left for Green and Airy, and extended in various ways by Stokes. It should not be overlooked that the work of both Cauchy and Poisson was only rendered possible by Fourier's analysis of an arbitrary function into simple-harmonic components. Not long afterwards Poisson took up the problem of the sound waves in an unlimited medium due to arbitrary initial conditions. The result is given in what Airy (I think) called the unsatisfactory form of a definite integral. The interpretation was not dwelt upon by Poisson, but here again had to wait for the penetrating genius of Stokes. It is then recognized that Poisson's formula, far from being unsatisfactory, gives precisely what one would wish to know, in the most convenient and appropriate form.

From this period onwards the flow of production was so rapid, and embraced so many subjects, that it is rather difficult to review it in any orderly sequence. One very important matter is the growth of the theory of Elasticity. The interest in this subject had been revived by the experiments of Chladni on vibrating plates, which formed a feature of the lectures on Acoustics which he gave in various places, as they have of most courses on the subject ever since. A skilled experimenter, and endowed with a fine musical ear, he was able not only to evoke a vast number of figures of nodal lines, formed by sand strewn on the plates, but also to assign their relative pitch, and even to formulate approximate numerical relations. His lectures were very successful, and appear to have excited the interest of the fashionable world, much as a lecture on soap-bubbles might at the present day. His visit to Paris was the occasion, at Napoleon's suggestion, that the theory of the figures now known by his name was proposed by the Academy as the subject of a prize essay for the year 1811. Among the competitors was one of the slender array of women who have figured in the history of Mathematics, Mdlle Sophie Germain. This lady had found inspiration in the pages of Montucla, and had devoted herself with great enthusiasm to the study of Mathematics, to the grievous distress of her parents. Lagrange, strange to say, had warned her that the problem was hopeless, and indeed her attempts were not very successful, even though she gained the prize at a subsequent competition. Like other of the earlier writers on the question, she assumed, on the analogy of Euler's problem of the bar, that the energy of deformation of a plate is a quadratic function of the principal curvatures. This is sufficiently correct, but the choice of the particular function was unfortunate. The further history of the problem is very interesting mathematically, but would lead us too far. The question could not be satisfactorily treated until the general theory of Elasticity had been further developed, and the relations between stresses and strains established. An additional impulse to the subject came from the wave-theory of Light which was growing rapidly at the hands of Young and Fresnel. The first essays at a general theory of elastic solids were made by Navier, Poisson, and Cauchy. Their investigations are noteworthy as including the first systematic attempts to deduce the properties of a body from the explicit hypothesis of a molecular structure. The word "molecule" it is true occurs over and over again in previous mathematical literature, but its meaning is usually that which we attach to the word "particle," viz. a small portion of a substance really treated as continuous. Laplace, again, had given a theory of capillarity based on the conception of forces having a very minute range of action, but the substance is treated as continuous, and the work was really a development of the theory of Attractions, with a generalized law of force. In the memoirs of Navier and

Poisson, and to a large extent in those of Cauchy, an elastic solid is conceived as a static arrangement of discrete molecules separated by finite intervals. The molecules are treated as mathematical points, and the mutual forces are supposed to be functions of the distance only, independent of direction. The range of the forces, though small, is assumed to be large compared with the intra-molecular spaces. All this is of course a possible conception, and a suitable matter for mathematical study, whether it corresponds to reality or not. One further assumption was, however, made, which has been much questioned, viz. that the displacements of consecutive molecules, when the body is deformed, are continuous functions of the co-ordinates. As applying to isotropic bodies in which the configuration of molecules about any point is assumed to be quite irregular, this can hardly be defended, but there is more to be said for it in the case of a crystalline structure. The continuity which is assumed in modern theories of Elasticity relates of course to averages, and not to individual molecules. Without further examination of the molecular assumptions, some of which are unnecessarily restricted, whilst the reasoning is sometimes difficult to follow, we may note that Navier and Poisson were led, in the case of isotropy, to equations which coincide with those generally accepted, except in one particular. The inference that there is an invariable ratio between the volume-elasticity and the rigidity of a substance was long a matter of controversy, but has not survived the criticism of Stokes and the experiments of Kirchhoff. Having obtained his equations, Poisson proceeds to apply them to various special problems, such as the radial vibrations of a sphere, the lateral vibrations of bars, and the symmetrical vibrations of circular plates. The latter especially is a skilful piece of analysis, involving Bessel Functions of both real and imaginary arguments, and is pushed to numerical results. The paper was soon followed by another, dealing with the problem of plane elastic waves in an isotropic solid. The two types characterized by longitudinal and transverse vibrations, respectively, are distinguished, and the corresponding wave-velocities found.

A great improvement in the theory was made by Cauchy, who initiated the modern theory of stress and strain. As an alternative to the method which he had first adopted, he abandons all explicit mention of molecules, and treats a solid as practically continuous. Extending the notion of pressure which was current in Hydrostatics, he assumes that the force between any two adjacent parts of a substance can be regarded as made up of actions between two strata of excessively small depth on the two sides of the interface, and may accordingly be treated as a surface-force or "stress." He goes on to investigate the relation between the stresses across different planes, and to express them geometrically by means of the stress-ellipsoid. This use of an ellipsoid to represent the relations between various directional properties in Mechanics is I believe original with Cauchy, who applied it also in the theory of strains, as well as in the more familiar matter of moments of inertia. His equations for an isotropic substance, obtained by this second method, are based on the hypothesis that the principal axes of stress and strain coincide, and have the now usual form, with two independent elastic constants. The whole procedure is in fact that found in modern books. It should be mentioned also that Cauchy in his work on strains introduces for the first time the notion of the infinitesimal rotation of an element, afterwards utilized by Stokes and Helmholtz.

Cauchy next took up the theory of crystalline solids, this time naturally on the basis of an assumed orderly arrangement of molecules, but his results have failed to stand the test of experiment, or to furnish a satisfactory explanation of double-refraction. The true theory of elastic solids in the general case, free from all molecular hypothesis, was given later by Green, whose work is the first example of the application of energy-methods to the physics of continua, the analytical process being an adaptation of the variational method of Lagrange. It is fortunately not my task to discuss these things from the point of view of Physical Optics, or to review the long-continued and obstinate attempts of successive physicists to construct a mechanical model of the ether, now definitely abandoned. At the present time the real outlet for the theory of elastic waves and their reflection and refraction is in relation to Seismology, where it has led to important results. The chief interest of the theory of Elasticity to us at the moment consists partly in the gradual emancipation from molecular assumptions, and partly in that the analytical relations which it involved were destined to find a wider and more important sphere of application. To take a very simple instance, in the equations of equilibrium of an incompressible isotropic solid,

$$\frac{\partial p}{\partial x} + \mu\nabla^2 u = 0, \text{ \&c., \&c.,}$$

$$\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y} + \frac{\partial w}{\partial z} = 0,$$

the symbols are such as present themselves in very different fields, and it is to be remembered that it was from this very example that Thomson, in his early speculations, constructed analogies between elastic displacements and rotations on the one hand, and distributions of electric and magnetic force in free space on the other.

To observe the growth of mathematical Electricity we must go back to the year 1811, when Poisson laid the foundations of Electrostatics as a branch of the theory of Attractions. Adopting the hypothesis of two electric fluids, he remarks that the resultant electric force at any point in the interior of a conductor must be zero. Combined with Coulomb's law of electric force, and Laplace's relation between normal force and surface density, this led at once to the distribution of electricity on a charged conductor in the form of an ellipsoid. Poisson further introduces the conception (but not the name) of the electric potential, and lays down the conditions which it has to satisfy at any point of the field due to a system of electrified conductors. In particular he investigates the induced distribution on a sphere due to any system of external charges. Finally, by a triumph of analytical skill, he solved the classical problem of two electrified spheres.

From the present point of view there is little further to record till Oersted's discovery of the action of an electric current on a magnetic needle (1820). This was followed almost immediately by Savart's analysis of the magnetic force into forces due to the infinitesimal elements of the electric circuit, and the simple rule which he formulated. This led Ampère to study the mechanical action between electric circuits. He analysed this into forces between the elements of the circuits, acting in the lines joining them, and subject to the law of action and re-action. His theory was based on a few plausible assumptions, and on a series of experiments devised in a strictly mathematical spirit to narrow down the various issues to be decided. His work is now seldom referred to, but it exhibits the mathematical skill which he had exercised before in the

Calculus of Variations, as well as in other directions. It is true that we are still in the atmosphere of action at a distance, and Ampère appeals in fact to the example of Newton and Gravitation, but only with Newton's qualification. He does not claim to have arrived at an ultimate explanation of phenomena, but only to have established a formula from which these can be calculated. The consequences which he deduced are more significant than the formula of elementary attraction itself. In the first place he finds that the resultant effect of a closed circuit on an element of another circuit depends on a vector which is afterwards identified with magnetic force. He then finds the force exerted on a small closed circuit, and proves it to be identical with the force on an elementary magnet. The familiar representation of a current by a magnetic shell follows, as well as the theory that the properties of a magnetized body are due to currents circulating in the molecules. Two provinces of physics, hitherto distinct, were here for the first time co-ordinated.

The views of Ampère, owing to their novelty, naturally excited at first some distrust. Preconceptions, especially when they have a definite form, die hard; and it is to be remarked that Poisson's great memoir on Magnetism, in which the hypothesis of two magnetic fluids is supposed to be verified, coincides almost in time with the latest publication of Ampère.

A good deal of Poisson's work on Magnetism has become classical, in the sense that subsequent writers have found nothing better than to reproduce it. It is largely independent of the two-fluid theory, and is really a theory of magnetic elements, afterwards treated explicitly as such, without further hypothesis, in the extensions given later by Thomson. The transformation by which the potential of a continuous arrangement of magnetic elements is expressed as due to distribution of imaginary magnetic matter through the volume and over the surface now appears for the first time. In his treatment of magnetic induction Poisson imagines his two fluids to be free to move within molecular spaces which for definiteness he treats as spherical. This latter assumption may be taken as merely illustrative, although it leads to a definite and sometimes impossible value of a coefficient. The particular problems solved, viz. the magnetization of a spherical shell, and of an ellipsoid, by a uniform field retain an interest independent of this special hypothesis.

The years which immediately followed were marked chiefly by the researches of Navier, Cauchy, and Poisson on Elasticity which have already been noticed. We come next to Green's Essay on Electricity and Magnetism (1828). The mathematical theory of Electrostatics, which had been initiated by Poisson, is here resumed and in a sense completed. The treatment is based on the theorem now generally quoted by the author's name. The novel point here is not the transformation from volume- to surface-integrals, for this was to be found in Poisson, but that it is the first example of the reciprocal relations which pervade not only Dynamics, but all branches of Physics. In the present case it is a relation between two different distributions of Electricity, but it only needs to give suitable meanings to the symbols to translate it into the language of Hydrodynamics or Acoustics. From the mathematical standpoint we have, further, the treatment of singularities of harmonic functions. The electrostatic theorems due to Green are reproduced in most modern text-books. Among original results we may notice the screening effect of conducting surfaces, the distribution of electricity on a spherical conductor due to internal or external charges, and the theory of condensers.

The phenomena of mutual induction and self-induction of electric currents were discovered by Faraday in 1831-35, but a long period elapsed before these received explicit mathematical investigation, and a longer still before it was recognized that Faraday's own description in terms of lines of force could be put in an exact mathematical form. The work of F. Neumann (1845–47) was the complement of that of Ampère and involved the same kind of ideas. The additional experimental fact adduced was Lenz's law. When there is relative motion of two circuits, or of a circuit and a magnet, currents are induced and there are consequent mechanical forces, which can be calculated from the formulae of Ampère. The law referred to is that the sense of the induced currents is such that these mechanical forces act in opposition to the relative motion. Neumann assumes this to be true also as regards the infinitesimal elements into which the circuits may be resolved, and further that the electro-motive force of induction is proportional to the velocity of the relative motion, to the strength of the inducing current or magnet, and to the component (with sign reversed) of the mechanical force in the direction of the relative motion. For the two former of these assumptions there was the experimental evidence of Faraday and others, the latter was adopted as the simplest supposition consistent with the law of Lenz. From this basis he proves that the total current induced in a circuit by the motion of a magnetic pole is proportional to the change in the potential of the pole in relation to a unit current in the circuit, and again to the change in the flux of magnetic force through the circuit. This is really Faraday's rule, except that it is not expressed in so many words in terms of lines of force. In the second paper he shews that the mechanical action between two currents depends on the mutual potential of the two circuits, viz.

$$\iint \frac{\cos \epsilon \, ds \, ds'}{r},$$

and refers the electro-motive forces of induction to changes in the value of this function.

We are still in the atmosphere of action at a distance, and it was therefore not unnatural that Weber and others should have looked for an explanation both of the mechanical and the inductive effects in a modification of Coulomb's law of force between electric charges. Since the actions to be explained depend on rates of change, violence had to be done to previous notions, and terms depending on mutual velocities and accelerations were introduced. The resulting law of Weber, which happened to be so framed as not to conflict with the conservation of energy, long exercised a fascination on continental writers, owing to the mathematical neatness of the processes by which the results of Ampère and Neumann could be deduced from it. It was not finally abandoned until Helmholtz shewed that under certain conditions it implied unstable electrical equilibrium, as well as other paradoxical consequences.

The year (1846) in which Weber's law of electric force was promulgated marks also very approximately the beginning of the modern tendency to ignore action at a distance, and to bring the medium across which electric and magnetic actions take place into the reckoning. The elastic analogies of Thomson have been mentioned already. Another analogy, between Electrostatics and Heat-Conduction, had been noted by him a little earlier, and used to illustrate various propositions in Attractions. The mathematical theory of Magnetism, next taken up by Thomson, was set forth in a form free from all hypothesis, the magnetic fluids of Poisson and others being now replaced by the notion of magnetic polarization. He further added to the grammar of continua by developing the conceptions and the properties of solenoidal and lamellar distributions of magnetism, which were suggested by Ampère's investigations. The two definitions of magnetic force in the interior of a magnet, afterwards distinguished as magnetic force and magnetic induction, are also introduced here for the first time. The whole memoir is a model of scientific exposition, and recalls the 'grand style' of the classical mathematicians, and especially of Gauss.

A final step towards a complete formulation on modern lines of the mathematical relations of Electricity consisted in the expression of magnetic force, or rather magnetic induction, in terms of the vector now known by the name of electric momentum. This vector, or its analogues, presented itself in various ways. We have first an investigation by Kirchhoff on the laws of induction in three-dimensional conductors, based on Weber's law of electric force. Almost simultaneously we have Stokes's paper on the Dynamical Theory of Diffraction, which is not so important nowadays as a contribution to Optics, but as containing a calculation of the waves in an elastic medium due to any initial disturbance. This was made to depend on Poisson's integration of the general equation of sound, and it is here that we meet for the first time with a full interpretation of this solution, which led up to that of the elastic wave-problem. The relation to the present matter consists, however, in the kinematical process by which displacements in any medium are expressed in terms of expansions and rotations, so that in Clifford's language everything is reduced to "squirts and whirls." The same process occurs again some years later in Helmholtz's great memoir on Vortex Motion, where we meet explicitly with the analogy of the relations between electric currents and magnetic force to those between vortices and fluid velocities. This analogy is developed towards the close of the investigation, but we can now see that it was implicit from the beginning in the very definition of a vortex. In both investigations the connection is established by means of a subsidiary vector, which in the electric analogy corresponds to the electric momentum of Maxwell.

The paper by Maxwell "On Faraday's Lines of Force," written shortly after he had taken his degree, is now perhaps little read, but deserves attention if only for the introduction, written in his own incomparable style, where we find already laid down the lines on which his subsequent speculations were to proceed. From the mathematical standpoint the paper is a comprehensive statement, without a suggestion of theory, describing the known facts of Electro-magnetism in terms of a system of vectors supposed to exist at all points of the field. Precision is here given to Faraday's idea of lines of force, whether electric or magnetic, by means of the analogy of the motion of an incompressible fluid. The new vector here introduced into Electromagnetism is that of momentum, and its rate of change is shewn by a dynamical argument to be responsible for electromagnetic induction. The proof of this depends on the expression for the energy of the field in terms of an integral extending over space, and is a deduction from the conservation of energy. The dynamical relation between ponderomotive and inductive forces had been indicated in a general way by Helmholtz in his celebrated tract, and this may possibly have been the first suggestion to Maxwell's subsequent dynamical theory.

The way was in fact now clear, so far as the mathematical scheme is concerned, for Maxwell's definite theory. He ventured as we all know to go a step further and to look behind the mathematical relations for a deeper insight into the matter, and if possible for a physical or mechanical meaning of the analytical symbols.

Regarding the question as a dynamical one he sketched out a mechanical model of the ether which should reproduce the known electrical relations, rather with a view of convincing himself that such a model was possible than as a definite explanation in detail. This was followed by the classical paper in which the laws of electro-magnetism were shewn to be deducible from dynamical considerations, without the assumption of any particular mechanism. The final presentment in his treatise, in which use is made of Lagrange's generalized equations, is too familiar to need further reference. Whether we prefer to regard it as an analogy or an explanation, it is a striking exemplification of the originality of Maxwell's genius.

At this point we may appropriately close our survey, for I do not undertake to be a guide in the subsequent history, which is still in the making. It is, however, to be remarked that Maxwell, who placed as it were the crown on one period of Mathematical Physics, was also in a sense the initiator of another, by his work on Gas Theory, which involved the creation of a molecular calculus.

Looking back on this long history we can trace through all the details an increasing tendency. The period we have been reviewing began under the influence of the great achievements of Laplace and Lagrange in the development of the Newtonian Astronomy. The notion of action at a distance, though not regarded by Newton himself as the last word on the matter, had had a great success, and when the field of Physical Astronomy was beginning to be fully occupied, the mathematicians who turned their attention to physical questions very naturally assumed that the same conception would be fruitful in other directions. Fortunately there was one physical process where these ideas obviously did not apply. Heat was indeed imagined to be a material, and moreover a fluid substance, but hardly molecular, and its transmission in conductors was naturally regarded as a continuous process. To this we owe the work of Fourier, which stands by itself, outside the historical order of development, except in so far as the solution of particular problems involved analytical processes, and led to analytical theorems which had a much wider scope. When the molecular structure of bodies was taken into account, as in the early days of Elasticity, the steps were somewhat vague and uncertain, and I think that the writers themselves must have experienced some relief when they had finally arrived at their differential equations, and felt really at home. It was a great improvement when the consideration of molecular forces could be dispensed with and replaced by Cauchy's theory of stress. The same tendency to discard unnecessary and unverifiable hypothesis has been exemplified in Electricity, in the transition from Poisson and Ampère to Thomson and Maxwell.

One feature which is met with in our period is the frank appeal to intuition. This is noticeable already in the case of Fourier, as has been already indicated, but it runs through the whole school. Even Cauchy, who was or became something of a purist according to the standards of his day, did not shrink on occasion from handling divergent integrals, but managed always to come right in the end. There is this to be said about mathematical work, in any but quite incompetent hands, that a too careless induction sooner or later betrays itself, and leads to a revision of the whole calculation. The great mathematicians, whatever licence they may have allowed themselves, have always had a sure instinct to save them from logical disaster. The *rôle* which intuition plays in mathematical discovery has sometimes been slighted or even denied. But was it not Gauss who, questioned as to the progress of a research on which he was engaged, replied that he had arrived at the theorems, and that it only remained to find the proofs? For such things as existence-theorems we must of course not look, at all events in the earlier half of our period. The first instance of the consciousness of such a requirement that I can call to mind occurs in Green, but he at once proceeds to appeal to physical conceptions. He wished to satisfy himself as to the existence of a function satisfying Laplace's equation, which should vanish over a closed surface, and have a definite singularity at a given internal point. He regards it as sufficient to remark that this is the case of an uninsulated conducting surface under the influence of an internal charge. The same use of physical proofs is to be found in Maxwell, and in an especial degree in the writing of the late Lord Rayleigh. The physical mathematician may reasonably claim a certain licence in this respect. He is often in the case of Gauss; the proposition is certain, but having his own business to attend to, he leaves the rigorous proof to the analyst, who ought indeed to be very grateful to him for the exquisite logical exercise which he has provided.

A further feature in the evolution is the gradual recognition of geometrical or physical meanings in various symbols or groups of symbols which are of constant recurrence. This is specially characteristic of the later stages. To Laplace and his school the potential was simply a convenient mathematical entity; the name with its associations came long afterwards from Green. The equation $\nabla^2\phi = 0$ lost most of its significance when it was transformed, as was necessary for some purposes, to polar co-ordinates, and the recognition of the general properties of the function was delayed. The equation itself first received an explicit interpretation at the hands of Maxwell, and the same holds with regard to the now familiar conceptions of 'divergence,' 'concentration,' and so on. And it needs hardly to be said that the notion of an operator, as distinguished from the result, belongs to the later period. The terminology of physical entities or qualities such as 'isotropy,' 'permeability,' and so on is largely due to Kelvin, with his copious onomastic faculty.

I have referred mainly to the development of general principles and methods, but that is, of course, not the whole of the story. A complete history would have to treat in some detail the special problems which suggested themselves from time to time. The impulse to general theory indeed often came about in this way. For instance, the problem of the two electrified spheres gave the impulse to Electrostatics, whilst Chladni's figures of nodal lines led up by degrees to the theory of Elasticity. It is, moreover, in the special applications that the skill of the analyst is particularly evoked, with results often of great interest and value even from the purely mathematical point of view. We need not go back to the theory of Attractions, or of the Figure of the Earth, which evoked Spherical Harmonics. The Conduction of Heat led incidentally to Bessel Functions, and above all to the theorems specially associated with the name of Fourier, whilst Poisson's problem of the two electrified spheres is a signal instance of the treatment of a functional equation. To Kelvin we owe the method of electric inversion, including the astonishing solution of the problem of the electrified spherical bowl, which had engaged the attention of Green, and the symmetrical treatment of Spherical Harmonics. To Maxwell are due the singularly beautiful solution of the problem of current sheets, a new interpretation of Spherical Harmonics, and other interesting results and points of view scattered through his treatise. As an example of a more systematic application of mathematical technique we may refer again to Cauchy's wave-problem, where the integrals afterwards attributed to Fresnel first make their appearance.

I have tried in this rapid sketch to do justice especially to the pioneers in the period; the merits and achievements of their more recent successors are fresh in our memories. It was I think fortunate that the first essays in the development of mathematical physics were by men whose accomplishments ranged over the whole of mathematics, and who thus had abundant analytical resources at their disposal. It may be claimed indeed that they provided almost the entire analytical equipment for their successors down to a comparatively recent time. You may search for instance the volumes of Lord Kelvin's papers and find hardly an appeal to any result of Pure Mathematics later than Cauchy, with the very important exception of what he had discovered himself. The most important province of later analysis which has found a direct application to physical questions is the Theory of Functions, and this again, so far as is necessary for the purpose, dates back to Cauchy, whom I should be disposed to place, after Fourier, as highest among the pioneers of mathematical physics.

I should like to be able to tell more about these men, about their characters, the vicissitudes of their lives and how these reacted on their work, their ambitions, their friendships, and even their quarrels and jealousies. Much that would be interesting is not to be found in official obituary notices. Sometimes an indication of these more human qualities has survived, such as the charming account of Ampère's early career, of the tragedy of his father's death in the Revolution, and of his idyllic love-story, and even the foible attributed to him in his later years, of carrying off in all innocence the wrong umbrella, even when there was no right one!

Some points of contrast with present conditions may be noted. The scientific work was largely academical, not so much that the men held as a rule official posts, or were trained in strict schools, but that they were under the influence of scientific Academies, which jealously guarded admission, and narrowly scrutinized the memoirs submitted to them. Consequently there was a tendency towards what I have called the 'grand style,' with great attention to form and presentation. One result is that their memoirs can often even now be referred to with interest, the absence of novelty in the subject matter being compensated by the literary charm.

But the great and I think the enviable point of difference is that there was little specialization, and no idea at all of a divorce between Pure and Applied Mathematics. The names I have so often had to quote testify how fruitful the alliance has been. And with all recognition of modern difficulties, I would quote the words of Fourier, but in a somewhat more catholic sense than he had in mind: "L'étude approfondie de la nature est la source la plus féconde des découvertes mathématiques."

The absence of English names from the first part of the record has often been remarked upon and deplored. The whole story and its lessons are given in Mr Rouse Ball's well-known *History of Mathematics at Cambridge*. We may point with pride however to the later achievements of our countrymen, most of them more or less connected with this University. Some features, specially characteristic, which we may claim as of English origin have been already indicated, the search for definite geometrical images of physical relations, and especially the cultivation of graphical methods. I may in particular mention the instructive diagrams which are appended to Maxwell's treatise, and which have been so great an assistance to the imagination of his readers, and so valuable as an example to later writers.

The period we have been surveying had I think a fairly definite beginning, and an almost equally definite close.

From the mathematical point of view its most striking achievement is the wide-embracing scheme of relations, which can be applied to so many diverse subjects, with hardly more than a change in the names of the various concepts. In their purely abstract form, in the rarefied atmosphere of Vector Fields, Triple Tensors, and so on, these relations might almost be developed in an hour, though they could hardly be understood or appreciated without reference to their physical aspects, to which they owe all their value. That such generality should have been attained is an instance of the constant endeavour of Mathematics to reduce to simple laws the infinite variety of nature. With a wider view than was possible to Fourier, we may echo his Newtonian quotation: *Quod tam paucis tam multa praestet geometria gloriatur.*

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